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Interactive comment on "Lack of ^{13}C-label incorporation suggests low turnover rates of thaumarchaeal intact polar tetraether lipids in sediments from the Iceland Shelf" *by* S. K. Lengger et al.

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We thank the reviewer for the thorough reading of our manuscript and the positive and constructive comments. We reply to the specific comments below.

Page 12809 Line 2-3: The references cited (Spang et al. 2013 and Brochier-Armanet et al. 2008) discuss the assignment of the Thaumarchaea as a separate phylum, not kingdom. Since "kingdom" is so rarely used now, I recommend avoiding it so as not to lead readers to mistakenly believe it is a new domain.

We will change this to phylum.

Line 6: Fuhrman et al. 1992 Nature should also be cited here.

We will insert this citation.

Line 11-12: Reword so as not to suggest there are no enrichment cultures.

We will reword this to make this distinction clear.

Page 12811 Lines 16-20. The authors mention in the abstract and suggest again here that the reason they have chosen these substrates is because they have been shown to be taken up by Thaumarchaea previously. I would caution the authors from assuming that all Thaumarchaea are metabolically identical, i.e. that if one takes up pyruvate they all do. In the discussion, the authors discuss the possibility that the archaea may not be able to consume the substrates provided, but it might be mentioned in the introduction, as well. It was unknown if the Thaumarchaea in the particular communities tested would be able to assimilate all (or any) of these substrates.

The reviewer is correct. We will modify the manuscript to state that there is a potential for great diversity of thaumarchaeal metabolisms, which has to be accounted for in investigations of their activity.

Page 12824 Lines 9-21: This is an excellent paragraph. Although the following section is interesting, these three options all remain a possible explanation for these results. Section 4.3 I am concerned about the calculation for growth rate. It assumes that all of the GDGTs are derived from living Thaumarchaea, which is not known. This assumption should be stated if this calculation is included. A small fraction of the total GDGTs may be highly enriched (derived from a community of fast growing Thaumarchaea) but not detected as such because of a large background of fossil GDGTs. To truly calculate the growth rate of the living community, the authors would need to know or determine the fraction of GDGT that is fossil versus within living cells. If all GDGT detected are indeed derived from living cells, then this growth rate is accurate, but otherwise it is an

underestimate.

We will include this assumption and make it clearer that this growth rate might be an underestimation if a large proportion of archaeal cells are fossil.

Page 12825 Lines 20-25: Providing the incubation times in these experiments would be useful.

We will provide these incubation times (24 h).

Page 12826 Lines 23-26: I don't believe cells from these depths would rupture at sea level pressure. Can the authors provide a reference?

There are indeed no studies on the stability of thaumarchaeal cells from these depths. However, following the recommendations of reviewer 1 we will carefully address the general differences between in situ and on board 'lab' experiments.

Page 12827 Line 1: "as well as" should be replaced by "and/or", as in the abstract, because as described above, Thaumarchaea growth may be fast, but simply appear slow due to the dampening of the isotopic signal by fossil GDGTs. Whether or not a small fraction of the community is fast growing or not, the overall turnover time of the bulk GDGTs remains very long, so the overall conclusion does not need to change.

This will be replaced by and/or.

Interactive comment on Biogeosciences Discuss., 10, 12807, 2013.